

specimens containing 40 per cent. of autunite, obtained direct from the mining syndicate, having been ground up together. From a fresh batch, obtained through a dealer, two single pieces were picked out, the first being an almost pure crystal weighing 2.3 grams, and of so fresh and new appearance that it looked as if it had been withdrawn from its mother-liquor but yesterday, and the second an obviously older looking, greener, and much larger mass containing 46 per cent. of matrix. The first gave a radium ratio of 70 per cent., and in it helium could not be detected. The quantity was not greater than 0.002 cu. mm. per gram U. This quantity would form in about thirty years! For the second, the radium ratio was 44 per cent. and the helium 0.035 cu. mm. per gram U, which would be produced in about 600 years. Lastly, Mr. Russell very kindly gave me the remains of the specimen for which he found 27 per cent. for the radium ratio. It weighed less than 0.5 gram, but the helium was easily detectable. It amounted to more than 0.15 cu. mm. per gram U, some being lost.

If these results are representative, the radium ratio decreases to a minimum and then rises more slowly as the helium content increases. If the latter is taken as a measure of the age of the mineral, the minimum appears to be reached after a few thousand years. This, of course, is exactly what would occur if, when the autunite was formed, the radium (but not its parent) associated with the uranium in its former condition separated with the latter. This in itself is not only possible, but probable, owing to the isomorphism of radium and calcium. But it is a somewhat startling result if initial radium can have any influence on the amount present in a mineral to-day, for this necessitates that the ages indicated by the helium content are not altogether below the truth, and that these beautiful crystals are actually even now in full process of formation.

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Stagnant Glaciers

IN the notice of the Professional Papers of the U.S. Geological Survey on the "Glaciers, Goldfields, and Landslides of North America," published in NATURE of July 21, attention is directed to the peculiar stagnant condition of some glaciers, and to the fact that certain glaciers, after being stagnant for long intervals, suddenly commence to move.

Although the movement of glaciers is such as would take place if they were viscous bodies, there is reason to believe that they have not all the same viscosity. I pointed out in a paper communicated to the Royal Society (Proc. Roy. Soc., 1908, p. 250) that the calculated viscosities of several Swiss glaciers varied from 292.2×10^{12} to 3.17×10^{12} C.G.S. units. Although some of the data upon which these figures were based were only estimated ones, I do not think that the different viscosities found are due wholly to errors in the data. In other words, that the viscosity of glacier ice is not a constant, as in the case of water, &c., but varies with variations in the granular structure of the ice, or that there is a limiting stress below which distortion does not take place as with plastic bodies.

So far as I am aware, no glaciers have been proved actually to be stagnant by careful measurement. Generally speaking, the conclusion that a glacier is dead is formed owing to the absence of certain features which are generally associated with glacier movement.

It is very desirable that such statements should be based upon actual measurements only, and also that the actual granular structure of the ice should be given, for there is every reason to believe that the viscosity of glacier ice varies with the size of the glacier grains. Were it not for the fact that the glacier grains are actually broken up by shear planes in the ice, they would gradually become larger and larger until they became so large, and the viscosity became so great, that the ice would scarcely move at all on small slopes. In such a case an earthquake might give rise to fractures in the ice, and by temporarily decreasing the viscosity increase the rate of flow.

R. M. DEELEY.

Melbourne House, Osmaston Road, Derby, July 23.

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It chanced, strangely enough, that Mr. Deeley's interesting letter reached me at a Norwegian port during the return journey of the Geological Congress party from Spitsbergen, on which Prof. R. S. Tarr, whose work has given rise to the letter, is a fellow-traveller with me. I have therefore taken advantage of the opportunity to discuss the subject with Prof. Tarr and other glacialists of our party.

Mr. Deeley is right in his supposition that the stagnant condition of the "dead ice" in Alaska has been inferred from surface indications, and has not yet been tested by actual measurement. It is, indeed, not likely that the ice of the areas described as "stagnant" is absolutely motionless, nor do I think that this has been implied in the descriptions. Such motion as it may have must however be very small, since it seems that the trees covering parts of the surface-moraines in the "dead" areas show no sign of disturbance.

As hinted in my review, it is evident that rapid advances of glaciers, comparable to those observed in Alaska, have taken place in regions where some other cause than an earthquake must be sought. During our recent journey in Spitsbergen, of which I hope shortly to give some account in these pages, we have been shown by our leader, Prof. G. de Geer, several cases of this kind which he has studied. It may be that Mr. Deeley's explanation of ice-structure will explain these rapid spasmodic movements, but I shall not venture upon a discussion of this difficult physical question. Mr. Deeley has at any rate suggested a line of research which ought to be followed up and experimentally tested in the field.

Stockholm, August 19.

G. W. LAMPLUGH.

The Leaning Tower of Pisa.

THE photograph of the "Leaning" Tower of Pisa in NATURE of August 4 shows clearly that the top tier is not square with the rest. From a rough alignment with the edge of a postcard, the photograph appears as if the tower was of the order of 25 mm./metre out of plumb when the top tier was put on presumably plumb.

Exact measures of this and of other parts of the tower might afford interesting data as to the epochs of the construction of the tower and of the progress of its "leaning."

EDWARD G. BROWN.

THIS famous tower will doubtless always be a question, like the man in the iron mask and other historical mysteries. Most architects, however, will be very slow to believe that it would have been built intentionally leaning on the general grounds that, however adventurous the architect, the clients would not have stood it. The analogy of the leaning towers of Bologna is hardly a sound one, as these plain shafts of brickwork, much like tall chimneys, can hardly be other than cases of settlement due to indifferently foundations. It should be remembered that construction was not a strong point with the Italians in the Middle and Renaissance Ages. In the case of the Tower of Pisa, Taylor particularly remarks on the wedge-shaped courses, which show an attempt to straighten the shaft. The best explanation appears to be that the tower was commenced, settled on its marshy bed, and that when the building was continued after a long interval it was considered safe to continue the work up to the limit of stability which could be calculated by the mathematicians of the epoch. The overhang is given by Taylor as 13 feet.

It is rather a pity that so much attention is concentrated by visitors on the tower, whereas the cathedral, Campo Santo, and particularly the Baptistery, are monuments of greater architectural importance. The design of the Baptistery is extremely interesting, and is perhaps the nearest expression of a Gothic dome.

The construction in this case is highly interesting, because the outer dome is supported by a cone, as at St. Paul's, London, but without an inner dome. As, however, the cone is not illuminated from the inside, it has a domical effect. The top of the cone shows externally, to the detriment of the general outline, not being cut off to carry a lantern as at St. Paul's.

Sir Christopher Wren may have known from travellers or by converse with foreign men of science of this example, but it is not necessary to jump to that conclusion, as an ordinary brick kiln or oast house would give the idea, aided by Wren's mathematical analysis of cones as units of high carrying power.

Taylor and Cresy's drawings of the Pisan monuments have every appearance of being most trustworthy, and should be consulted by your correspondent. I had the plates with me when visiting Pisa in 1890, and I had the opportunity to go up the tower and round its galleries. Ruskin has a passage on the setting out of the lower part of the western façade of the cathedral, but I remember the impression produced by my examination was not favourable to his argument.

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The Origin of the Domestic "Blotched" Tabby Cat.

THE question of the origin of the two types of our domestic cats has been the subject of much controversy, and it is therefore with diffidence that the views here expressed are now put forward. It is, of course, well known that any domestic "tabby" can, at a glance, be assigned to one of the two colour patterns, "striped" or "blotched."

In a recent paper (Proc. Zool. Soc., 1907, pp. 143-66) Mr. R. I. Pocock comes to the conclusion that the origin of *F. catus* (blotched tabby) is "at present quite unknown," and suggests that it is "the survivor of some extinct, probably Pleistocene, cat of Western Europe" (*ibid.*, p. 160); in effect, he regards *catus* as a good species. It seems to have been pretty clearly shown by the same writer that the *torquata* breed (striped tabby) is either the direct descendant of *F. sylvestris* or is the result of a cross between that species and *F. ocreata* (Proc. Zool. Soc., 1907, p. 947, and NATURE, vol. lxxvii., p. 414), which latter is, no doubt, merely a geographical race of *sylvestris*.

In his previous paper (Proc. Zool. Soc., 1907, p. 160) Mr. Pocock remarks that "when two distinct species cross the hybrid sometimes reverts in some respects to the characters of a [supposed] common ancestor of both"; this cannot be denied, but such a cross more commonly results in a form intermediate between the two parents, usually designated as a mongrel. After much diligent search, I have been unable to find a single instance in which complete segregation has taken place in respect of all specific characters when two well-defined species are crossed.

The two "types" of tabby, when crossed, always produce individuals which are at once referable to one or the other variety; in short, we get complete (Mendelian) segregation in respect of this character.

It therefore seems to me to be incompatible with the above observed facts, that *F. catus* is the survivor of some extinct cat of Western Europe, for if *catus* were a good species, when crossed with *torquata* we would most certainly have some form of intermediate produced. This, as we know from everyday experience, is contrary to the expressed results of such a cross. From these facts it is suggested as a possible explanation that *F. catus* arose *per saltum* from *F. sylvestris*. In short, I believe that *F. catus* has arisen from *F. sylvestris* as a "sport," and when crossed with its parent species or *inter se* follows the Mendelian law of segregation, as many such discontinuous variations have now been proved to do. At the same time (from evidence which cannot be here brought forward), it would appear that only in extremely rare cases, if at all, can Mendelian action be accountable for the evolution of a species in nature.

In opposition to such an origin, Mr. Pocock urges (Proc. Zool. Soc., 1907, p. 160) "the complete absence of evidence that species of *Felis* are ever dimorphic in pattern, and the ascertained fact that they breed true to their specific and sub-specific type." The objection, of course, is a purely negative one, and there is some evidence to show that animals under domestication are more subject to pronounced variation than in a state of nature.

In the leopard (*F. pardus*) we have a species of *felis*

which can most certainly be regarded as dimorphic, in that it produces a black form, and (so far as the somewhat meagre information on the subject goes) in its gametic behaviour is exactly comparable to the case of the "blotched" and "striped" tabby. There are, so far as I know, no data in the case to show which is the "dominant" form, but, from analogy, it is almost certain the black would be dominant over the spotted. It is the hope of obtaining such information in the case of our common cats which has induced me to approach the subject. Finally, it may be said that, although no direct proof can be brought forward in support of such a suggestion, I am convinced that a properly conducted series of experiments with the two types would bring to light much evidence in favour of such a view.

Unfortunately, the writer is at present unable to carry out such a series of experiments, and it is hoped that others may hereby be induced to do so.

H. M. VICKERS.

81A Princes Street, Edinburgh, August 20.

I AM glad Mr. Vickers has directed the attention of Mendelians to the question of our two types of "tabby" cat. With the same purpose in view, and in the hope of inducing someone with time and facilities at his disposal to carry out breeding experiments with these animals, I recently communicated to the Mendel Society a paper on this subject, which will appear in the forthcoming issue of the journal. The results of such experiments are sure to be interesting, but whether or not they will settle the origin of the "blotched" tabby is another matter. They may turn the balance of the evidence in favour of this or that theory, but it is doubtful if they will result in more than a hypothetical conclusion. For myself I have quite an open mind on the point. As stated in my original paper on English cats, the "blotched" tabby may be regarded provisionally either as a survivor of some extinct cat that formerly inhabited Europe or as a "mutation" of the "striped" tabby. I reserved the names "*catus*" and "*torquata*" for these two types as a convenient means of designating them, following Linnaeus's method, which is still in vogue, of assigning a specific epithet to our domesticated animals, like *Ovis aries*, *Canis familiaris*, and others, when their origin is uncertain or unknown.

I think Mr. Vickers a little overstates the case when he says there has been much controversy on the subject of the origin of these cats, and speaks of their existence as well known. It was the fact that the remarkable differences between them had been practically ignored or unappreciated by zoologists that induced me to discuss the question at some length three years ago. Nor do I think Mr. Vickers himself quite appreciates the distinction I emphasised between dimorphism in pattern and dimorphism in colour. Experience with wild animals shows that pattern is far more stable than colour. Pattern is wonderfully persistent; colour is not. No one would be greatly surprised at finding a black or white example in a litter of spotted hyænas, but it would be admittedly an extremely remarkable thing if a specimen resembling a striped hyæna in pattern occurred amongst them. Such a "mutation" would be comparable to the "mutation," if mutation it be, of the "blotched" from the "striped" tabby cat. Such a mutation in pattern as that supposed in the case of the hyæna may, of course, be produced to-morrow; but, so far as I am aware, no such variation has as yet been recorded, and I write this with full recollection of the curious variations in pattern that have been recorded of the common leopard.

Finally, may I demur to one more statement made by Mr. Vickers, namely, that animals under domestication are more subject to pronounced variation than those in a state of nature? I do not dispute this common assumption, but I am not satisfied that the evidence in its favour amounts to very much.

The questions raised by Mr. Vickers are, however, full of interest; and all that I have said is in justification of the agnostic attitude that I think should be, for the present, preserved towards the origin of the "blotched" tabby cat.

R. I. Pocock.

Zoological Gardens, August 24.